Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2017-183-AC3, 2017

Atmospheric Measurement Techniques Discussions

Interactive comment on "Intra-urban spatial variability of surface ozone and carbon dioxide in Riverside, CA: viability and validation of low-cost sensors" *by* Kira Sadighi et al.

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Received and published: 10 December 2017

Reviewer 2: This manuscript describes the deployment of low-cost ozone and CO2 monitors in Riverside, CA. Many groups are working on these sorts of low-cost sensors, but to date most papers have focused on calibration and sensor characterization. As the authors note in the introduction, few papers outline deployment of sensors to quantify variations in air quality. Therefore, this manuscript's description of a deployment of ∼10 sensors represents a meaningful contribution to the field.

*Thank you for your input on this manuscript. After more consideration, we have decided to remove the CO2 analysis portion from the paper, and to make the ozone

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analysis stronger. As such, many of the comments addressing CO2 have not been answered completely, as those sections are no longer included. The specific comments laid out below have been addressed with an asterisk here, as well as in the text, where appropriate.

-Section 2.2 - The field calibration needs more explanation. What are the coefficients p? Why is the functional form of the calibration for CO2 and O3 different? I am aware that this group has written previous papers on sensor calibration, but not all readers will be familiar with that work. A more thorough, stand-alone description of the calibration method is required. * A more through stand-alone description of the sensors and the calibration technique was added for clarity. The discussion of the coefficients has been addressed from reviewer 1.

-The comparison of calibration versus deployment performance is useful (e.g., Figures 5 and 6), as these illustrate that differences in O3 measured during the deployment period are "real". However, given that Fig 7 shows that most of the U-Pods are correlated, Figure 6 would be more effective if the raw (rather than absolute) concentration difference was shown. *For this analysis, we wanted to determine if there was spatial variability. The absolute value told us how different the sites were. The next step could be to use the raw concentration to determine the directions of difference, but as an initial try, that method is more complex and was difficult to clearly display for the reader.

-For cases in Fig 7 where deployment data cluster around the 1:1 line - how can authors be sure that this is true variation and not some sort of uncertainty? One might expect less scatter around the 1:1 line for training data than for deployment data. E.g., if 50% of the calibration data is randomly held out as a test set, what would these scatter plots look like for the held out portion of the co-location period? *This question is not completely clear to us. We think you are saying that there could be more scatter in the blue data if we only used 50% of the data. A similar test to this was performed with the validation dataset. For D0 and D7, the validation dataset was iterated 200 times

with 10% of the data each time to address this issue. Validation results suggest our measurement uncertainty is between 4.4 and 5.9 ppbv and scatter more than this is variability.

-Figure 8 - I do not understand the point of this figure, and it needs to be discussed more thoroughly. My read on it is that D3 is systematically lower than D7 at all times of day. *That is correct, that D3 is lower at all times of day. It is different than all the other U-Pods, and therefore worth discussing. We have fleshed out this discussion to explain why that might be the case and go in more depth with hour-of-the day analysis.

-Figure 9 - Do all of the data forming the "claw shape" come from the same day? Or was this phenomenon observed across a number of days? *They come from three groups of a few consecutive hours, similar to the claw effect observed in D7. These claws are independent of the validation claw seen for D7 in the first week of validation.

-The discussion of CO2 needs to be better integrated into the manuscript. At times it reads like the manuscript was written for ozone, and CO2 was an afterthought. *Analysis of CO2 has been omitted to further investigate at a later date.

-Page 3, Line 4 - Where does the information on the number of monitors required for Riverside- San Bernarndino come from? * Corrected by responding to referee 1, this information is provided by the US EPA and codified in the code of federal regulations.

-Page 4 - Line 2 says that the study area was 314 kmˆ2 but line 4 and Fig 1 suggest a much smaller area. Please clarify. *Thank you for pointing this out. A circle that encompasses all the pods except DD has a radius of ∼8km, about 200 kmˆ2. This was the resultant area used for the analysis.

-The discussion of auto-ranging for CO2, and how it was dealt with, are confusing. I cannot tell if that data was removed or somehow corrected. If corrected, what was the procedure? * Analysis of CO2 has been omitted to further investigate at a later date.

-The manuscript has both an Appendix and an SI, which seems redundant. The plots

in the Appendix should either be in the main text or in the SI. * We have removed the Appendix and moved plots to the SI.

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